Atmos. Chem. Phys. Discuss., 8, S9521–S9526, 2008
www.atmos-chem-phys-discuss.net/8/S9521/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S9521–S9526, 2008

Interactive Comment

Interactive comment on "Synoptic influences on springtime tropospheric O_3 and CO over the North American export region observed by TES" *by* J. Hegarty et al.

Anonymous Referee #2

Received and published: 28 November 2008

I have a fundamental problem with the major premise of this manuscript. The authors assume that if they composite six different patterns of sea level pressure, that distinct patterns of CO and O3 will result. This approach does not consider the different higher altitude flow patterns that can occur within a given cyclone composite. This approach also does not consider directly how the various airstreams are located with respect to sources of pollution. The result is that the CO and O3 patterns for the six classes do not have clearly different patterns, and the authors have difficulty explaining these chemical patterns. There is a great deal of speculation about what leads to each pattern (WCB, location of pollution sources, etc.). And, the manuscript’s summary barely mentions the six categories, but instead focuses on the overall six category composite along with a few specifics. I get the impression that the authors have attempted to cover too much material (overall seasonal, specific map types, individual cases), with most of the discussions turning out to be unconvincing.

I would have approached this topic differently. I would begin with cases of TES-derived patterns of CO and O3 related to East Coast mid latitude cyclones, calculate backward trajectories from specific chemical features within each, and then group the trajectories based on the sources (or lack of sources) that were encountered. There may be other approaches that would yield a similar result.

Unless the authors can convince me that their approach is valid, I cannot recommend publication of this manuscript. Specific issues are noted below.

1. page 19748, line 28 and elsewhere—“681 O3” This is a poor grammatical way to express O3 at 681 hPa. It is a type of lazy grammar.

2. top of page 19750—It is not clear why you used a universal a priori field. Specifically your sentence, “The geographically variable a priori adds artificial structure, which can potentially obscure some of the real geographical variability of a trace gas.” I thought one purpose of the a priori was to help the retrieval process produce variability that it otherwise would not detect because of limited vertical resolution in the sensor and the retrieval algorithm. I don’t understand your thinking here. Please explain it better in the text.

3. page 19751 line 12—Please better describe HYSPLIT’s ensemble approach—specifically the “shifting of the met fields by one grid point”. Does this amount to picking a location and then creating multiple trajectories around it and in the vertical?

4. page 19751 line 6—When I have used the Lund (1963) technique, I required a correlation of at least 0.7. Did you investigate whether requiring a greater correlation would produce better results? Keep in mind that it is better to leave some cases uncat-

ACPD

8, S9521–S9526, 2008

egorized than to have too many variations within a given category. How many of your cases were left uncategorized?

Lines 10-15—Although the GPH upper level fields are “smoother and less distinct” that SLP”, subtle differences in upper level flow play a crucial role in determining transport. This gets back to my opening paragraph. Your assumption on lines 14-15 is a HUGE one, and I do not believe it is sufficiently valid.

5. Fig.2—These panels would be much easier to interpret if you would label each center with an H or an L.

6. page 19753— Although this section attempts to distinguish between the various map types, I do not believe that I could duplicate your categorizations because your criteria do not appear “hard and fast”. Instead, there appears to be considerable subjectivity in the categorizations e.g., in distinguishing MAMS 2-5. Please describe your criteria more specifically, perhaps by modifying Table 1 or adding a new table. The more “hard and fast” your criteria are, the greater your chances of having CO and O3 patterns that are explainable.

If I am correct, the unique feature of MAM1 is its semi-stationary nature. However, I note on page 19765 that you labeled a case persisting from 9-15 May as MAM3. This confuses me. Also by first discussing the various airstreams when describing MAM2, you give the impression that MAM1 does not have these airstreams (i.e., WCB, etc.) Was that your intention? Doesn’t MAM1 also have these airstreams in most cases?

You describe types MAM2-5 in the last two paragraphs of Section 3 and refer to Fig. 2. Some of the MAMs look very similar in Fig. 2—just some small displacements. So, as you point out, their histories were very important in the classification process. Nonetheless, even cyclones having the same track do not necessarily have the same vertical structure such that their trajectories would be the same.

ACPD

8, S9521–S9526, 2008

Interactive Comment

Finally you mention that the CCB often is a very cloudy region. However, the same can be said of the WCB where it overrides the CCB in the vicinity of the warm front. In fact, the classical airstream model shows greatest WCB ascent in the warm frontal area

7. Throughout the text you use “elevated” in places where I think you mean “enhanced”. The text would be more specific if you reserved “elevated” to references to altitude. This especially is problematic in those sentences that describe both altitude as well as concentration.

8. Fig. 3—MAM6 has the greatest PV. Any thoughts as to why this occurs? MAM1 has an even lower SLP, while MAMs 2 and 4 have the same central pressure (1004 hPa). Of course, SLP alone is not the sole indicator of PV, but that gets back to my major concern with your methodology.

9. page 19757 line 13—There is no Fig. 7g; it should be Fig. 6g.

10. Tables 1 and 2 and corresponding text—What was your total number of cases? Also, it would be helpful if the total for each map class would be added as a column in Table 1. Table 2 has them listed by regions, but an overall total for each MAM would be useful.

11. I have no problem with Fig. 4 and its discussion. It is the individual map types that follow that are a concern.

12. page 19756—Please succinctly state your reasons and criteria for defining Regions 1-3. You state that Region 1 contains the greatest CO, but what were the criteria for defining the other two regions?

Later when you describe the individual map types in Table 2 and the text, the lat/lon bounds for the three regions do not vary with map type. So, for example, do the lat/lon bounds for MAM1 always contain the greatest CO—even though the low centers are not at the same locations? This confuses me.

13. There is a great deal of speculation (“likely a result of”, “sug-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

gests”, “may be”, “may have potentially”, etc.) about the causes for the various CO and O3 features in the six map types. I suppose this is the best you can do based on your categorization methodology, but that gets back to my major objection to this approach.

14. page 19759 line 17 and Fig. 8—At 316 hPa, doesn’t one expect that the northern third of your domain will be dominated by the stratosphere (whether or not a cyclone is present)? This will have a major influence on your correlations. Shouldn’t this be mentioned?

According to classical cyclone theory, the WCB will not have transported air to the 316 hPa level until considerably north of the warm front’s surface position. This would generally occur northeast of the low center. The bottom of page 19759 does not make this point clear.

15. page 19760 line 18— “the cyclones were displaced from the coast by anticyclones”. This is not good meteorological wording. Reading farther, why should this result in significant contributions from the WCB? The DA contribution is easier to understand since more of the DA region is within your lat/lon domain, but I do not understand the WCB aspect. It is very difficult to determine your major points in Section 4.3

16. Section 4.4—The center of the surface low in MAM6 appears to be farthest out to sea of any other map type. Doesn’t this fact alone have a major influence on the chemical concentrations and patterns?

17. Last para. of Section 4.4—This is a very telling paragraph. Assuming that the manuscript can be made viable, this information also needs to be stated at the beginning of the results sections

18. page 19763—Your first case study represents a fairly deep, but compact cyclone. Yet, the IR image does not reveal a major cloud band associated with the WCB. Adding

ACPD

8, S9521–S9526, 2008

a VIS image to Fig. 10 might be helpful here since it hopefully would show the lower level portion of the WCB. It would not require extra space since you currently have a gap where the fourth panel would be placed.

19. page 19764—Based on Fig. 11, it appears that approximately half of your trajectories might represent the secondary WCB due to their path and low level origin. However, the other half appears to begin at a high altitude and begin northwest of the low center. This suggests a dry intrusion. The fact that they passed over large emissions while approaching the low and associated frontal region could explain the CO and O3. So…you might have a combination of the two airstreams. This is interesting and could be presented in greater depth.

20. Section 6 has a weak start. Please provide a better theme sentence.

21. page 19765 line 25—I am still puzzled as to why a cyclone lasting from 9-15 May should be categorized MAM3 instead of MAM1

22. Section 6—I believe your goal here is to present a case of enhanced O3 over the Atlantic in the lower troposphere that suggests continued O3 production. However, I am not sure what your final conclusion is. Your trajectories are interesting, but what have you proven? It appears that much more is needed.

I wish you the best with revising this manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 19743, 2008.

ACPD

8, S9521–S9526, 2008

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper